is the main object of the Committee. A large amount of information is already in hand, much of which has been supplied by Mr. J. B. Redman, who for many years has devoted special attention to this subject. Mr. R. B. Grantham has also made important contributions respecting parts of the south-eastern coasts. But this information necessarily consists largely of local details, and it has been thought better to defer the publication of this for another year. Meanwhile the information referring to special districts will be made more complete, and general deductions may be more safely made. As far as possible the information obtained will be recorded upon the six-inch maps of the Ordnance Survey. These give with great accuracy the condition of the coast, and the position of every groyne, at the time when the survey was made.

Appended is a copy of the questions circulated. The Committee will be glad of assistance, from those whose local knowledge enables them to answer the questions, respecting any part of the coast-line of England and Wales. Copies of the forms for answering the questions can be had on application to the Secretaries.

Appendix—Copy of Questions.—1. What part of the English or Welsh coast do you know well? 2. What is the nature of that coast? (*a*) if cliffy, of what are the cliffs composed? (*b*) what are the heights of the cliff above H.W.M.? Greatest, average, least. 3. What is the direction of the coast-line? 4. What is the prevailing wind? 5. What wind is the most important—(*a*) in raising high waves? (*b*) in piling up shingle? (*c*) in the travelling of shingle? 6. What is the set of the tidal currents? 7. What is the range of tide? Vertical in feet, width in yards between high and low water, at spring tide, and at neap tide. 8. Does the area covered by the tide consist of bare rock, shingle, sand, or mud? (*a*) its mean and greatest breadth; (*b*) its distribution with respect to tide-mark; (*c*) the direction in which it travels; (*d*) the greatest size of the pebbles; (*e*) whether the shingle forms one continuous slope, or whether there is a "spring full" and "neap full," if the latter, state their heights above the respective tide-marks. 10. Is the shingle accumulating or diminishing, and at what rate? 11. If diminishing, is this due partly or entirely to artificial abstraction (*see* No. 13)? 12. If groynes are employed to arrest the travel of the shingle, state—(*a*) their direction with respect to the shore-line at that point; (*b*) their length; (*c*) their distance apart; (*d*) their height—(1) when built, (2) to leeward above the shingle, (3) to windward above the shingle; (*e*) the material of which they are built; (*f*) the influence which they exert. 13. If shingle, sand, or rock is being artificially removed, state—(*a*) from what part of the foreshore (with respect to the tidal range) the material is mainly taken; (*b*) for what purpose; (*c*) by whom—private individuals, local authorities, public companies; (*d*) whether half-tide reefs had, before such removal, acted as natural breakwaters. 14. Is the coast being worn back by the sea? If so, state—(*a*) at what special points or districts; (*b*) the nature and height of the cliffs at those places; (*c*) at what rate the erosion now takes place; (*d*) what data there may be for determining the rate from early maps or other documents; (*e*) is such loss confined to areas bare of shingle? 15. Is the bareness of shingle at any of these places due to artificial causes? (*a*) by abstraction of shingle; (*b*) by the erection of groynes, and the arresting of shingle elsewhere. 16. Apart from the increase of land by increase of shingle, is any land being gained from the sea? If so, state—(*a*) from what cause, as embanking salt-marsh or tidal foreshore; (*b*) the area so regained, and from what date. 17. Are there "dunes" of blown sand in your district? If so, state—(*a*) the name by which they are locally known; (*b*) their mean and greatest height; (*c*) their relation to river mouths and to areas of shingle; (*d*) if they are now increasing; (*e*) if they blow over the land, or are prevented from doing so by "bent grass" or other vegetation, or by water channels. 18. Mention any reports, papers, maps, or newspaper articles that have appeared upon this question bearing upon your district (copies will be thankfully received by the Secretaries). 19. Remarks bearing on the subject that may not seem covered by the foregoing questions. [N.B.—Answers to the foregoing questions will in most cases be rendered more precise and valuable by sketches illustrating the points referred to.]

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE

On Loss of Heat by Radiation and Convection as affected by the Dimensions of the Cooling Body; and on Cooling in Vacuum, by J. T. Bottomley.—In the course of a series of experiments on the heating of conductors by the electric current, which were carried on during the past winter, I obtained a considerable number of results which both gave me the means of calculating the *emissivity* for heat in absolute measure of various surfaces under different circumstances, and also caused me to undertake a number of special experiments on the subject. These experiments are still in progress, and I am making preparation for a more extended and complete series; but a brief notice of some of the results already arrived at may not be without interest to the British Association.

The experiments were made on wires of various sizes, some of them covered and some of them bare, cooling in air at ordinary temperatures, and at normal and also at very much reduced pressures.

The mode of experimenting was as follows;—

A current passing through a wire generates heat, the amount of which is given by Joule's well-known law—

$$H = C^2 R/J \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where C is the current, R the electrical resistance, J Joule's equivalent, and H the quantity of heat generated per unit of time; each being reckoned in C.G.S. units. Let l be the length of the wire, d its diameter, and σ_t the specific resistance of the material at temperature $t°$ (at which temperature let us suppose that the wire in the given external conditions is maintained by the current). Then

$$R = \frac{\sigma_t l}{\frac{1}{4}\pi d^2} = \frac{4\sigma_t l}{\pi d^2}$$

Hence from (1)—

$$H = \frac{C^2}{J} \cdot \frac{4\sigma_t l}{\pi d^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Consider, now, that the wire suspended in the air is losing heat by its surface, and let us suppose that it neither loses nor gains heat by its ends. Let H' be the quantity lost by emission from the surface per unit of time. Let e be the emissivity, or quantity of heat lost per unit time per unit area of the cooling surface per unit difference of temperatures between the cooling surface and the surroundings; and $t°$ being, as has been said above, the temperature of the wire, let θ be the temperature of the surroundings. Then

$$H' = \pi d l . e . (t - \theta) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

But when the wire has acquired a permanent temperature, with the current flowing through it, there is as much heat being lost at the sides as is being generated by the current. In this case $H = H'$; and we obtain the expression for e—

$$e = \frac{4 C^2 \sigma_t}{J \pi^2 d^3 (t - \theta)} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

My experiments consist in measuring the strength of the current and the temperature of the wire, the latter being effected by measuring the electric resistance of a known length of the wire while the current is flowing through it, and hence inferring the temperature. These being known, and likewise the temperature of the surroundings, we have all the data for finding e, the emissivity of the surface in absolute measure. The experiments of Mr. D. Macfarlane giving emissivities in absolute measure are well known, and are of undoubted accuracy. They were communicated to the Royal Society (*Proc.* Roy. Soc., 1872, p. 93); and the results are quoted in Prof. Everett's "Units and Physical Constants" (chap. ix. § 137). These experiments were made with a copper globe about 4 cm. in diameter, suspended in a cylindrical chamber, with top and bottom, about 60 cm. in diameter, and 60 cm. high. The results may be briefly summed up as follows:—

Macfarlane finds an emissivity of about 1/4000th of the thermal unit C.G.S. per square centimetre per second per degree of difference of temperatures between cooling body and surroundings for a polished surface, with an excess of temperature of a little more than 60° C.; and, for a blackened surface, the same emissivity with an excess of 5° C. or under.

Using round wires of small diameter (0·85 mm. and under), and with the surfaces either brightly polished or in common dull condition of a wire fresh from the maker, I have found a much larger emissivity than 1/4000. I have obtained different values of e for wires of different sizes, varying from 1/2000 down to 1/400, which was obtained with a wire of 0·40 mm. diameter, and with an excess of temperature of 24° C. It seems to be shown by all the experiments I have made that, other things being the same, the smaller the wire the greater the emissivity.

To do away with the part of the emissivity which is due to convection and conduction by the air, I have commenced experiments on loss of heat by small wires in the nearly perfect vacuum afforded by the modern mercurial air-pump. This part of the subject was experimented on long ago by Dulong and Petit, and within the last few years by Winkelman and Kundt and Warburg; lastly, and much more perfectly, by Mr. Crookes (*Proc.* Roy. Soc., vol. xxxi. p. 239), though in no case, I believe, were the emissivities in absolute measure determined. The conclusion come to by all these experimenters is the same, namely, that there is a decrease of emissivity due to lowering of the air-pressure, this decrease being very small for a reduction down to one-half or one-third of the ordinary atmospheric pressure, but becoming very great as the vacuum approaches completeness.

The very interesting experiments of Mr. Crookes seem to show that, even with the high vacuum which he obtained, the effect of the residual gas in carrying off heat from the cooling body was far from being annulled.

The following table shows the emissivity of a copper wire with bright surface half a metre long, 0·40 mm. in diameter, and sealed into a glass tube about 1·5 cm. in internal diameter:—

Current in amperes	Pressure, 760 millims.		Pressure, 380 millims.		Pressure, 180 millims.		Very high vacuum pump worked continuously	
	$t-\theta$	e	$t-\theta$	e	$t-\theta$	e	$t-\theta$	e
1	4°·7	1/1822	4°·5	1/1784	5°·5	1/2176	17°	1/6443
2	22·5	1/2084	21·5	1/1996	23.5	1/2174	68	1/5620
3	56	1/2114	58	1/2180	55	1/2082	140?	1/4606[1]

The following table may also be found interesting. It shows the emissivity in absolute measure of some materials commonly used as insulating coverings for wires.

Specifying number B.W.G. and nature of covering	Length of wire. Centims.	Resistance of 100 centims. B.A. units	Diameter of wire in millims.	Diameter of covered wire, outside measurement	Current (amperes)	$t-\theta$	Emissivity
No. 22, silk-covered ...	100	·0395	·76	·96	10	23°·4	0·001333
No. 26, cotton-covered	100	·094	·50	·88	10	58	0·001385
No. 26, silk-covered ...	100	·1115	·45	·57	9·8	70	0·002020
No. 22, gutta-percha...	100	·0455	·72	1·67	10	24	0·000854
No. 22, tinned, gutta-percha-covered, and double cotton covered outside	100	·0432	·73	1·86	10	23	0·000759

On Some Phenomena Connected with Iron and other Metals in the Solid and Molten States, with Notes of Experiments, by W. J. Millar, C.E., Sec. Inst. Engineers and Shipbuilders in Scotland. —1. *Object of Paper.*—Results of experiments by the author with various metals, such as cast-iron, gun-metal, phosphor-bronze, lead, copper, and type-metal. The object being to determine the cause of the well-known phenomena of the flotation of cold cast-iron on molten cast-iron, and as to whether any expansion took place upon solidification in the metals above noted. 2. Notes of some of the experiments from which the author concludes that the cause of flotation of the solid metal on liquid metal of the same kind is *buoyancy*, due to expansion suddenly set up in the immersed pieces, and that this expansion was found by careful measurement to be at least equal to the shrinkage or total decrease in length of the piece from white hot solid to finally cooled down solid. Further, that the expansion observed is obtained within much lower limits of temperature than the shrinkage; as the pieces, which were in all cases removed from the molten metal, immediately on appearing floating hardly showed redness, and when broken it was found that the crystalline character of the metal remained. 3. Notes of experiments made by gradually heating pieces of cast-iron—the results of all these experiments leading the author to conclude that the *rate* of expansion in cast-iron is at first much more rapid at low temperature than afterwards at high temperature. 4. From experiments carried on with pieces of lead and copper and type-metal, it was found that if any flotation occurred it was only with small light pieces—heavy pieces sinking and remaining at bottom of ladle. Gun-metal and phosphor-bronze behaved like cast-iron. 5. Consideration of the peculiar appearance, or "*break*," observed on the surface of molten cast-iron, the figures presenting a geometrical pattern, like interlacing circles or stars. The author believes that this appearance is due to cracks forming upon the rapidly forming skin—these cracks taking more or less a circular form from the convex forms into which the various parts of the surface are thrown, due to the bubbling up of gas or air. This appearance is limited to cast-iron, and experienced observers can tell the quality of the iron from the form of pattern or figures showing on the molten surface. 6. From observation and experiments carried out from time to time, the author concludes that no perceptible increase of volume of the metals noted occurs at the moment of solidification; at least when free from air or gas confined within the casting.

On a Gyrostatic Working Model of the Magnetic Compass, by Sir William Thomson.—In my communication to the British Association at Southport,[1] I explained several methods for overcoming the difficulties which had rendered nugatory, I believe, all previous attempts to realise Foucault's beautiful idea of discovering with perfect definiteness the earth's rotational motion by means of the gyroscope. One of these, which I had actually myself put in practice with partially satisfactory results, was a

Gyrostatic Balance for Measuring the Vertical Component of the Earth's Rotation.

It consisted of one of my gyrostats supported on knife-edges attached to its containing case, with their line perpendicular to the axis of the interior fly-wheel and above the centre of gravity of the fly-wheel and framework by an exceedingly small height, when the framework is held with the axis of the fly-wheel and the line of knife-edges both horizontal, and the knife-edges downwards in proper position for performing their function. The apparatus, when supported on its knife-edges with the fly-wheel not spinning, may be dealt with as the beam of an ordinary balance. Let now the framework bear two small knife-edges, or knife-edged holes, like those of the beam of an ordinary balance, giving bearing-points for weights in a line, cutting the line of the knife-edges as nearly as possible, and of course (unless there is reason to the contrary in the shape of the framework) approximately perpendicular to this line, and, for convenience of putting on and off weights, hang, as in an ordinary balance, two very light pans by hooks on these edges in the usual way. Now, with the fly-wheel not running, adjust by weights in the pans if necessary, so that the framework rests in equilibrium in a certain marked position with the axis of rotation inclined slightly to the horizontal, in order that the axis of the fly-wheel, whether spinning or at rest, may always slip down so as to press on one and not on the other of the two end plates belonging to its two ends. Now, unhook the pans and take away the gyrostat and spin it; replace it on its knife-edges, hang on the two pans, and find the weight required to balance it in the marked position with the fly-wheel now rotating rapidly. This weight, by an obvious formula which was placed before the Section at Southport, gives an accurate measure of the vertical component of the earth's rotation.[2]

Gyrostatic Model of the Dipping Needle

I also showed at Southport that the gyrostatic balance described above, if modified by fixing the knife-edges, with their line passing as accurately as possible through the centre of gravity of the fly-wheel and framework, and with the faces of the knives so placed that they shall perform their function properly when the axis of the fly-wheel is parallel to the earth's axis of rotation, and the rotation of the fly-wheel in the same direction as the earth's, will act just as does an ordinary magnetic dipping needle; but showing latitude instead of dip, and dipping the south end of the axis downwards instead of the end that is

[1] Temperature probably much too low. The wire, sagging down, touched the inside of the glass tube in several points.

[1] No report of this communication has, so far as I know, hitherto appeared in print.

[2] The formula is

$$gw = \frac{1}{a} Wk^2\omega\gamma \sin l;$$

where w denotes the balancing weight; gw the force of gravity upon it; a the arm on which this force acts; W the weight of the fly-wheel; k its radius of gyration; ω its angular velocity; γ the earth's angular velocity; and l the latitude of the place.

towards the north as does the magnetic dipping needle. Thus, if the bearing of the knife-edges be placed east and west, the gyrostat will balance with its axis parallel to the earth's axis, and therefore dipping with its south end downwards in northern latitudes and its north end downwards in southern latitudes. If displaced from this position and left to itself, it will oscillate according to precisely the same law as that by which the magnetic needle oscillates. If the bearings be turned round in azimuth, the position of equilibrium will follow the same law as does that of a magnetic dipping needle similarly dealt with. Thus, if the line of knife-edges be north and south, the gyrostat will balance with the axis of the fly-wheel vertical, and if displaced from this position will oscillate still according to the same law; but with directive couple equal to the sine of the latitude into the directive couple experienced when the line of knife-edges is east and west. Thus this piece of apparatus gives us the means of definitely measuring the direction of the earth's rotation, and the angular velocity of the rotation. These experiments will, I believe, be very easily performed, although I have not myself hitherto found time to try them.

Gyrostatic Model of a Magnetic Compass

At Southport I showed that a gyrostat supported frictionlessly on a fixed vertical axis, with the axis of the fly-wheel horizontal or nearly so, will act just as does the magnetic compass, but with reference to "astronomical north" (that is to say, rotational north) instead of "magnetic north." I also showed a method of mounting a gyrostat so as to leave it free to turn round a truly vertical axis, impeded by so little of frictional influence as not to prevent the realisation of the idea. The method, however, promised to be somewhat troublesome, and I have since found that the object of producing a gyrostatic model of the magnetic compass may, with a very remarkable dynamical modification, be much more simply attained by merely suspending the gyrostat by a very long, fine wire, or even by floating it with sufficient stability on a properly planned floater. To investigate the theory of this arrangement let us first suppose a gyrostat, with the axis of its fly-wheel horizontal, to be hung by a very fine wire attached to its framework at a point, as far as can conveniently be arranged for, above the centre of gravity of fly-wheel and framework, and let the upper end of the wire be attached to a torsion head, capable of being turned round a fixed vertical axis as in a Coulomb's torsion balance. First, for simplicity, let us suppose the earth to be not rotating. The fly-wheel being set into rapid rotation, let the gyrostat be hung by the wire, and after being steadied as carefully as possible by hand, let it be left to itself. If it be observed to commence turning azimuthally in either direction, check this motion by the torsion head; that is to say, turn the torsion head gently in a direction opposite to the observed azimuthal motion until this motion ceases. Then do nothing to the torsion head, and observe if a reverse azimuthal motion supervenes. If it does, check this motion also by opposing it by torsion, but more gently than before. Go on until when the torsion head is left untouched the gyrostat remains at rest. The process gone through will have been undistinguishable from what would have had to be performed if, instead of the gyrostat with its rotating fly-wheel, a rigid body of the same weight, but with much greater moment of inertia about the vertical axis, had been in its place. The formula for the augmented moment of inertia is as follows. Denote by—

W, the whole suspended weight of fly-wheel and framework;
K, the radius of gyration round the vertical through the centre of gravity of the whole mass regarded for a moment as one rigid body;
w, the mass of the fly-wheel;
k, the radius of gyration of the fly-wheel;
a, the distance of the point of attachment of the wire above the centre of gravity of fly-wheel and framework;
g, the force of gravity on unit mass;
ω, the angular velocity of the fly-wheel: the virtual moment of inertia round a vertical axis is

$$\mathrm{WK}^2\left(1 + \frac{w^2 k^4 \omega^2}{\mathrm{W}^2 \mathrm{K}^2 a g}\right) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The proof is very easy. Here it is. Denote by—

ϕ, the angle between a fixed vertical plane and the vertical plane containing the axle of the fly-wheel at any time t;
θ, the angle (supposed to be infinitely small and in the plane of ϕ), at which the line a is inclined to the vertical at time t;
H, the moment of the torque round the vertical axis exerted by the bearing wire on the suspended fly-wheel and framework.

By the law of generation of moment of momentum round an axis perpendicular to the axis of rotation requisite to turn the axis of rotation with an angular velocity $d\phi/dt$, we have

$$wk^2\omega\frac{d\phi}{dt} = g\mathrm{W}a\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

because $g\mathrm{W}a\theta$ is the moment of the couple in the vertical plane through the axis by which the angular motion $d\phi/dt$ in the horizontal plane is produced. Again, by the same principle of generation of moment of momentum taken in connection with the elementary principle of acceleration of angular velocity, we have

$$wk^2\omega\frac{d\theta}{dt} + \mathrm{WK}^2\frac{d^2\phi}{dt^2} = \mathrm{H} \quad . \quad . \quad . \quad . \quad (3)$$

Eliminating θ between these equations we find

$$\left(\frac{w^2 k^4 \omega^2}{g\mathrm{W}a} + \mathrm{WK}^2\right)\frac{d^2\phi}{dt^2} = \mathrm{H} \quad . \quad . \quad . \quad (4)$$

which proves that the action of H in generating azimuthal motion is the same as it would be if a single rigid body of moment of inertia given by the formula (1), as said above, were substituted for the gyrostat. Now to realise the gyrostatic model compass: arrange a gyrostat according to the preceding description with a very fine steel bearing wire, not less than 5 or 10 metres long (the longer the better; the loftiest sufficiently sheltered inclosure conveniently available should be chosen for the experiment). Proceed precisely as above to bring the gyrostat to rest by aid of the torsion head, attached to a beam of the roof or other convenient support sharing the earth's actual rotation. Suppose for a moment the locality of the experiment to be either the North or South Pole, the operation to be performed to bring the gyrostat to rest will not be discoverably different from what it was, as we first imagined it when the earth was supposed to be not rotating. The only difference will be that, when the gyrostat hangs at rest, relatively to the earth, θ will have a very small constant value; so small that the inclination of a to the vertical will be quite imperceptible, unless a were made so exceedingly small that the arrangement should give the result, to discover which was the object of the gyrostatic model balance described above; that is to say, to discover the vertical component of the earth's rotation. In reality we have made a as large as we conveniently can; and its inclination to the vertical will therefore be very small, when the moment of the tension of the wire round a horizontal axis perpendicular to the axis of rotation of the fly-wheel is just sufficient to cause the axis of the fly-wheel to turn round with the earth. Let now the locality be anywhere except at the North or South Pole; and now, instead of bringing the gyrostat to rest at random in any position, bring it to rest by successive trials in a position in which, judging by the torsion head and the position of the gyrostat, we see that there is no torsion of the wire. In this position the axis of the gyrostat will be in the north and south line, and, the equilibrium being stable, the direction of rotation of the fly-wheel must be the same as that of the compotent rotation of the earth round the north and south horizontal line, unless (which is a case to be avoided in practice) the torsional rigidity of the wire is so great as to convert into stability the instability which, with zero torsional rigidity, the rotational influence would produce in respect to the equilibrium of the gyrostat with its axis reversed from the position of gyrostatic stability. It may be remarked, however, that even though the torsional rigidity were so great that there were two stable positions with no twist, the position of gyrostatic unstable equilibrium made stable by torsion would not be that arrived at: the position of stable gyrostatic equilibrium, rendered more stable by torsion, would be the position arrived at, by the natural process of turning the torsion head always in the direction of finding by trial a position of stable equilibrium with the wire untwisted by manipulation of the torsion head. Now by manipulating the torsion head bring the gyrostat into equilibrium with its axis inclined at any angle, ϕ, to that position in which the bearing wire is untwisted; it will be found that the torque required to balance it in any oblique position will be proportional to the sin ϕ. The chief difficulty in realising this description results from the great augmentation of virtual moment of inertia, represented by the formula (1) above. The paper at present communicated to the Section contains calculations on this subject, which

throw light on many of the practical difficulties hitherto felt in any method of carrying out gyrostatic investigation of the earth's rotation, and which have led the author to fall back upon the method described by him at Southport, of which the essential characteristic is to constrain the frame of the gyrostat in such a manner as to leave it just one degree of freedom to move. The paper concludes with the description of a simplified manner of realising this condition for a gyrostatic compass—that is to say, a gyrostat free to rotate about an axis either rigorously or very approximately vertical.

Section C—Geology

On Ice-Age Theories, by Rev. E. Hill, M.A., F.G.S., Tutor of St. John's College, Cambridge.—On the Montreal Mountain, in the neighbouring quarries, at the mouth of the Saguenay River, and more or less everywhere over all Canada and all the north and north-west of this continent, are seen phenomena which imply a former vastly extended action of ice. The like are found over Europe and Asia, thus completely encircling the Pole. Many theories have been propounded to account for these facts. It is proposed to pass these before you in review. Any explanation ought to account not only for cold greater than the present, but for accumulations of snow and ice. A kindred phenomenon is the greater size of the Antarctic ice-cap. The supposed interglacial warm periods, and the unquestioned luxuriance of Miocene vegetation in Greenland, ought also to find their causes in any thoroughly satisfactory theory. The theories which have been propounded fall into three groups, as Cosmical, Terrestrial, and Astronomical (or Periodical). The Cosmical theories are Poisson's Cold-Space theory—incomprehensible; and the Cold-Sun theory of S. V. Wood and others—lacking any evidence. The Terrestrial theories are numerous. Lyell's suggestion of Polar-continent and Equatorial-ocean is opposed by evidence that continents and oceans lay on much the same areas as now. The contrary view, Polar-ocean and Equatorial-land, would deserve consideration but for the same opposing evidence. The elevation view (Dana, Wallace), which alleges greater altitude of mountain-chains, disagrees with the strong evidence for land-depression during the period. The submergence view of Dr. Dawson agrees with this evidence, but requires elucidation. Alteration of ocean-currents (Gunn, J. S. Gardiner) is a most powerful agency, but would act locally rather than universally round the Pole. Alteration of prevalent winds, hitherto worked out by no one, deserves attentive consideration. Conditions are conceivable which would produce over an area winds from cold quarters almost permanently. However, this seems open to the same objection as the preceding theory. Last come the Astronomical or Periodical theories. A tilt of the earth's axis was suggested by Belt, but suggested as owing to causes which are wholly insufficient. Tilting from astronomical agencies is slight, though its action would be in the direction required. Herschel suggested the Eccentricity theory, but abandoned it. Adhémar's Precession theory, as explained by himself, involved an absolute fallacy. The celebrated view of Dr. Croll combines the Precession and Eccentricity theories into one. It exactly agrees with the Antarctic greater extension of ice, and provides an explanation of interglacial warm periods. The great difficulty in its way is to see how a mere difference in distribution through the year of an unchanged total heat-receipt can produce consequences so vast. The laws of radiation explain but a very minute part, the laws of evaporation perhaps rather more; but, so far as can at present be seen, both together are inadequate. Another serious objection is that the theory seems to require the climate of the northern hemisphere to be now in a state of change for the better, of which at present there appears no evidence. Dr. Croll's elaborate explanations of the reaction of one effect upon another—fogs, deflection of currents, and the like—have no special connection with his own theory. They would act in all cases, and support all theories equally. The arguments, if admitted, would only prove that the earth's climates are in a state of highly unstable equilibrium, in which a slight cause may produce an enormous change. Nor are his arguments universally admitted. In conclusion, Dr. Croll's theory seems inadequate: alteration of currents and winds are the most powerful causes suggested hitherto: further investigations ought to be made as to the nature and extent of the last series of changes in the outlines of the continents of the globe.

What is a Mineral Vein or Lode? by C. Le Neve Foster, B.A., D.Sc., F.G.S., H.M. Inspector of Mines.—The author quoted briefly the definitions of a mineral vein given by Werner, Carne, Von Cotta, Grimm, Von Groddeck, Geikie, Sandberger, and Serlo, who, in common with most geologists, have looked upon mineral veins as "the contents of fissures." While admitting that a very large number of veins may be so described, the author contended that the exceptions are sufficiently important and numerous to warrant a change in the definition. He is of opinion that many of the principal and most productive tin-lodes in Cornwall are simply tabular masses of altered granite adjacent to fissures; and he brought forward the opinions of other geologists to show that certain veins in the English Lake district, the Tyrol, Nova Scotia, Nevada, Colorado, California, and Australia, are not filled-up fissures. In conclusion, he proposed the following definition: "A mineral vein or lode is a tabular mineral mass formed, more or less entirely, subsequently to the inclosing rocks."

The Acadian Basin in American Geology, by L. W. Bailey, Geological Survey of Canada.—The Acadian Basin, embracing the region bordering on and including the Gulf of St. Lawrence, together with the provinces of New Brunswick, Nova Scotia, Newfoundland, and Prince Edward Island, constitutes one of the natural physical divisions of the continent of North America, and exhibits many marked peculiarities of climate and floral and faunal distribution. In its geological structure, and in the history which this reveals, its individuality is not less clearly marked, being often in strong contrast with that of other portions of the continent farther west; and in some periods and features even exhibiting a closer relationship with the geology of Europe. In the present paper, the facts bearing upon this individuality are summarised and discussed; including the consideration of the varying land-surfaces of Acadia in different eras, the time and nature of its physical movements, its climate, and its life. A review of recent progress in the investigation of its geological structure is also given.

Upon the Improbability of the Theory that former Glacial Periods in the Northern Hemisphere were due to Eccentricity of the Earth's Orbit, and to its Winter Perihelion in the North, by W. F. Stanley, F.G.S., F.R.Met.S.—The theory of Dr. Croll, accepted by many geologists, is that former glacial periods in the northern hemisphere were due to greater eccentricity of the earth's orbit, and to this hemisphere being at the time of glaciation in winter perihelion. This theory is supported upon conditions that are stated to rule approximately at the present time in the southern hemisphere, which is assumed to be the colder. Recent researches by Ferrel and Dr. Hann, with the aid of temperature observations taken by the recent Transit of Venus expeditions, have shown that the mean temperature of the southern hemisphere is equal to, if not higher than the northern, the proportions being 15·4 southern and 15·3 northern. The conditions that rule in the south at the present time are a limited frozen area about the South Pole, not exceeding the sixtieth parallel of latitude; whereas in the north frozen ground in certain districts, as in Siberia and North-Western Canada, extends beyond the fiftieth parallel; therefore by comparison the north, as regards the latitude in which Great Britain is situated, is at present the most glaciated hemisphere. As it is very difficult to conceive that the earth had at any former period a lower initial temperature, or that the sun possessed less heating power, glaciation in the north could never have depended upon the conditions argued in Dr. Croll's theory. The author suggested that glaciation within latitudes between 40° to 60° was probably at all periods a local phenomenon depending upon the direction taken by aërial and oceanic currents; as, for instance, Greenland is at present glaciated, Norway has a mild climate in the same latitude, the one being situated in the predominating northern Atlantic currents, the other in the southern. Certain physical changes suggested in the distribution of land would reverse these conditions and render Greenland the warmer climate, Norway the colder.

On the Occurrence of the Norwegian "Apatitbringer" in Canada, with a few Notes on the Microscopic Characters of some Laurentian Amphibolites, by Frank D. Adams, M.Ap.Sc., Assistant Chemist and Lithologist to the Geological Survey of Canada.—The paper first gives a short account of the investigations which have been made on this amphibole-scapolite rock in Norway, where all the principal deposits of apatite either traverse it or occur in its immediate vicinity. The deposits of apatite in Canada generally occur associated with some variety of highly pyroxenic rock, often holding orthoclase and quartz. The "Apatitbringer" has, however, recently been found in the